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EDWIN DUNKIN, Esq., F.R.S., Vice-President, in the Chair.

John McCance, Esq., Rathfern, Rayners Road, Putney Hill, was balloted for and duly elected a Fellow of the Society.

On some Systematic Errors in the Determinations of the Semi-diameter of the Moon from the Greenwich Observations 1750–1840. By E. J. Stone, M.A., F.R.S.

The longitudes of the Moon's centre which are contained in the Greenwich Lunar Reductions, vol. ii., appear to be affected by considerable systematic errors directly connected with the reduction of the observations from the limbs to the centre. The excess of the observed longitudes of the centre over the tabular longitudes are given separately for each limb whenever both limbs are observed near the Full. I have extracted these quantities for groups of years identical with those adopted by Sir G. B. Airy for the determination of the coefficient of the parallactic inequality; and if E_1 and E_2 denote these excesses of observed longitudes from observations of the first and second limbs respectively, the mean values of $\frac{E_1 - E_2}{2}$ will be found, in my table, under the heading δs. I also give the values of the coefficient of the parallactic inequality deduced directly by Sir G. B. Airy, and their values when approximately corrected for the errors of semi-diameter δs.

Period.	Parallactic Inequality with adopted Semi-diam.	Correction. δs	Parallactic Inequality apparently corrected.
	″	″	″
1750–1759	121·44	+0·15	121·60
1755–1764	121·27	+0·29	121·58
1760–1768	122·15	+0·37	122·54
1765–1773	122·63	+0·14	122·78
1769–1778	122·87	+0·50	123·40
1774–1782	123·23	+0·65	123·92
1779–1787	122·34	+2·23	124·69
1783–1791	121·15	+2·05	123·31
1788–1796	121·06	+1·03	122·15
1792–1801	121·03	+0·95	122·03
1797–1805	120·49	+1·01	121·56
1802–1810	121·17	+0·38	121·57
1806–1815	122·98	+0·01	122·99
1811–1819	124·09	−0·50	123·56
1816–1824	124·49	−0·01	124·48
1820–1829	125·82	+0·26	126·09
1825–1833	125·20	+0·54	125·77
1830–1838	124·01	+1·05	125·11
1834–1842	123·57	+0·30	125·11
1839–1847	123·24	+0·40	123·88
1843–1851	124·52	+0·70	123·67
1847–1851	125·50	+0·87	125·26
1851–1861	124·38	+0·63	125·01
1862–1876	124·76	+0·40	125·18

The result for 1851–1861 is the mean of the results for the two nine-year periods 1851–1859 and 1853–1861.

The Altazimuth Observations give

1848–1861	″125·64
1862–1878	125·06.

The adopted mean semi-diameters are stated by Sir G. B. Airy to have been, $0{\cdot}273036 \times 3421''{\cdot}8$, or $15'\ 34''{\cdot}27$, for the observations 1750 December 12, to 1815 August 19; and quantities sensibly agreeing with $0{\cdot}27294 \times 3421''{\cdot}8$, or $15'\ 33''{\cdot}95$, for the observations wth the New Transit from 1816 August 7 to 1839 December 31. From the year 1840 to 1861 values agreeing sensibly in mean results with Adams's value, $15'\ 34''{\cdot}68$, appear to have been adopted; whilst in and after 1862 Hansen's value, $15'\ 34''{\cdot}08$, has been adopted. An inspection of the values of δs shows at once that the forty-six-year period inequality,

whose existence was first pointed out by Sir G. B. Airy in the values of the coefficient of the parallactic inequality deduced from a discussion of these observations, and which has recently been brought prominently before the Society as an inequality in the expression of the Moon's tabular longitude, is certainly directly connected with questions of semi-diameter. But it does not appear that any inequality with the same period and to the same amount is shown by our recent determinations of semi-diameter or coefficients of parallactic inequality.

The corrections δs for the groups 1779–1787 and 1783–1791 are perhaps unduly increased by the presence in both groups of an error, $7''{\cdot}6$, which was found on 1874 Sep. 28. But if this observation should be rejected, the values of δs would still remain $1''{\cdot}82$ and $1''{\cdot}65$ respectively, and the general law would therefore remain unchanged.

It appears to me most desirable that some competent person should re-examine the calculations of the parallaxes and semi-diameters which were actually used on the work prior to 1840, when Adams's parallax was practically adopted by the application of the necessary corrections to Burckhardt's values.

There is another point of view under which the collection of these values of the coefficient of the parallactic inequality may not be without interest. The value of the mean diameter of the Moon, near the Full, found from the observations with the Old Transit 1750–1815, does not greatly differ from the value found with the present Transit-Circle. It might therefore have been expected that the coefficient of the parallactic inequality determined from these early observations would not differ very much from the value given by the recent observations. But this is not the case. The coefficient given by the observations 1750–1815 is only $122''{\cdot}6$, a value which, curiously enough, differs but little from that which would be found if Encke's value of the solar parallax were adopted; but the value given by the recent observations is about $125''{\cdot}2$. The difference is serious, and cannot be dismissed as due to mere personality or as certainly due, without examination and proof, to differences of instrumental means; for the diameters determined with the different instruments have been used in passing from the observed limbs to the centre, and the results should therefore be free from error of semi-diameter.

But there are indications that the numerical values of the coefficient of the parallactic inequality increase as the optical power of the instrument employed on the observations is increased. If such is the case, the effective semi-diameters near Full Moon must sensibly differ from those near quadratures. There is undoubtedly one cause for such differences in instrumental irradiation; but it is very doubtful whether this can be the principal cause of the discordance, $2''{\cdot}6$, to which I have called attention. If such were really the case, we should expect the semi-diameter found from a discussion of the observations

with the small instrument to be much larger than with the present Transit-Circle; and this is not the case. There is one cause which has occurred to me as a possible explanation of the observed discrepancy. The irregularities of the lunar surface subtend very sensible angles at the Earth; and these irregularities must present different aspects on the limbs when viewed under the different illuminations at Full and at quadrature; and the measured differences would most probably differ considerably with such very different instrumental means as those employed 1750–1815 and those in present use. There is an old transit instrument, with an aperture of an inch and a half, at the Radcliffe Observatory. I feel much tempted to mount the instrument and try whether there really does exist a difference between the effective semi-diameters near Full and quadrature sufficient to account for a discrepancy of $2''\cdot6$ in the values of the coefficient of the parallactic inequality as determined from observations with such an instrument and our present Transit-Circle. If such a discrepancy could be proved to exist, we should be able to infer with certainty the existence of some smaller correction in the same direction for our existing instruments; but if no such discrepancy should be found between the results, then there must be some very important error in the discussion of the early Greenwich results to have led to this constant difference.

On the Conjunctions of the Satellites of Uranus with each other, which may be observable from February to May 1882. By A. Marth, Esq.

The following list is a continuation of that printed on p. 29, and contains the computed position-angles and distances, in reference to the centre of *Uranus*, of the satellites which pass one another, for the nearest even hours, Greenwich M.T., preceding and following the times of their conjunctions.

		Ariel.		*Umbriel.*		*Titania.*		*Oberon.*	
	G.M.T.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
1882.	h	°	″	°	″	°	″	°	″
Feb. 1	12	—		192·6	13·0	198·7	17·2	—	
	14	—		193·2	14·9	199·3	15·4	—	
	16	—		193·7	16·7	200·0	13·5	—	
15	16	195·8	13·9	—		190·1	9·9	—	
	18	196·2	12·3	—		191·0	11·9	—	
	20	196·8	10·2	—		191·7	13·8	—	
18	14	—		194·9	21·0	196·6	23·8	—	
	16	—		195·1	21·3	196·8	22·2	—	
	18	—		195·3	21·1	197·1	20·5	—	

		Ariel.		*Umbriel.*		*Titania.*		*Oberon.*	
	G.M.T.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.	Pos.	Dist.
1882.	h.	°	″	°	″	°	″	°	″
Feb. 20	12	195·0	15·2	—		—		190·6	13·1
	13	195·1	15·3	—		—		190·9	13·9
	14	195·3	15·2	—		—		191·2	14·8
	15	195·4	14·9	—		—		191·4	15·6
21	10	13·3	8·6	15·3	11·3	—		—	
	12	14·0	11·2	15·4	8·9	—		—	
27	16	193·2	7·2	196·9	10·4	197 1	16·1	—	
	18	193·9	9·8	197·6	8·0	197·4	14·2	—	
	20	194·3	12·0	199·0	5·4	197·8	12·2	—	
	22	194·6	13·7	—		198·4	10·3	—	
March 8	18	14·8	13·9	14·3	13·7	—		—	
	20	14·9	14·9	14·5	15·6	—		—	
9	16	—		15·4	18·6	13·9	15·4	—	
	18	—		15·5	17·1	14·0	17·2	—	
10	14	195·9	8·3	193·5	7·0	—		—	
	16	196·5	5·4	194·0	9·5	—		—	
12	18	—		14 2	9·8	16·0	15·6	13·6	14·1
	20	—		14·4	12·1	16·2	13·7	13·8	15·8
	22	—		14·5	14·2	16·4	11·7	13·9	17·5
17	8	194·7	8·0	—		195·8	10·2	—	
	10	194·8	10·5	—		196·0	8·2	—	
19	18	—		195·0	21·2	—		194·7	19·5
	20	—		195·0	20·8	—		194·7	21·2
21	8	—		14·9	17·0	15·2	18·3	—	
	10	—		15·0	18·4	15·2	16·5	—	
23	18	—		—		195·0	32·4	195·0	34·8
	20	—		—		195 0	33·1	195·0	33·6
	22	—		—		195·0	33·7	195·0	32·3
26	8	15·0	12·8	14·9	16·9	—		15·2	16·5
	10	15·0	14·2	14·9	15·2	—		15·1	18·2
	12	15·0	15·0	14·9	13·2	—		15·1	19·8
29	8	14·8	12·4	15·4	9·9	—		—	
	10	14·7	10·3	15·3	12·2	—		—	
April 4	14	194·7	13·4	195·7	10·8	—		—	
	16	194·6	11·7	195·5	13·0	—		—	
8	12	14·3	10·3	—		—		17·6	10·2
	14	13·9	7·7	—		—		17·2	12·0
9	8			195·0	21·0	195 9	19·3	—	

	G.M.T.	*Ariel.* Pos.	Dist.	*Umbriel.* Pos.	Dist.	*Titania.* Pos.	Dist.	*Oberon.* Pos.	Dist.
1882.	h.	°	″	°	″	°	″	°	″
April 9	10		—	194·9	21·1	195·7	21·0	—	
	12		—	194·8	20·9	195·6	22·7	—	
13	8	14·7	14·4		—	17·1	11·5	—	
	10	14·5	13·1		—	16·7	13·4	—	
	12	14·2	11·3		—	16·4	15·3	—	
	18		—		—	16·0	20·6	13·4	20·9
	20		—		—	15·8	22·2	13·2	19·3
19	16		—		—	194·7	33·8	194·1	34·6
	18		—		—	194·7	33·3	194·0	33·4
	20		—		—	194·6	32·7	193·9	32·2
20	10	16·1	9·5	13·5	11·8		—		—
	12	15·7	11·7	13·0	9·5		—		—
23	8	14·8	14·9	16·2	13·3		—		—
	10	14·6	14·3	15·9	15·2		—		—
25	10	16·5	8·3		—	12·1	12·3		—
	12	15·9	10·7		—	11·5	10·4		—
26	12		—	193·8	14·9	197·4	13·8		—
	14		—	193·5	13·0	197·0	15·7		—
	16	196·6	7·9	193·1	10·8		—		—
	18	196·0	10·4	192·4	8·5		—		—
May 4	8	196·0	10·9		—		—	190·8	13·1
	10	195·6	12·8		—		—	190·1	11·4

The observations of these conjunctions may serve, not only as test-observations for the theories of the motions of the satellites, but also as tests to show the power of the most powerful telescopes of the present day; and, as such conjunctions will not occur again till the year 1923, it is to be hoped that the opportunities of the coming season will not be neglected. I need scarcely add that this is also the best time for settling the question whether a fair determination of the ellipticity of *Uranus* is or is not within the reach of the best micrometrical measurements.

On the Motion of the Companion of Sirius.

By W. E. Plummer, Esq.

The history of this interesting system is too well known to need any mention here, but the recent publication in a collected form of the accurate series of observations made at the Washing-

ton Observatory makes this a fitting and easy opportunity to examine whether the observed companion is moving in an orbit resembling that in which, according to the discussion of Dr. Auwers, the perturbing body must move.

The deviation of the observed coordinates, more especially of position-angle, from those predicted by Dr. Auwers from the discussion of the proper motion of *Sirius*, has thrown some doubt on the generally received opinion of the identity of the observed companion with the perturbing body in the *Sirius* system, and recently this doubt has been somewhat strengthened by M. Flammarion, who in 1877 wrote "L'orbite apparente observée croise dès 1869 l'orbite apparente calculée et se projette en dehors suivant une toute autre courbe, qui sera plus vaste et moins excentrique."*

The deviations of the computed orbit from the observed positions made at Washington during the last few years are exhibited in the Table I. given below; and one of the objects of the present communication is to inquire how far M. Flammarion is justified in the conclusion he has drawn from these and similar data at his disposal.

Table I.

Date.	Observed Pos. Angle.	C−O	Observed Distance.	C−O
	°	°	″	′
1874·23	58·05	+7·08	11·11	−0·19
75·28	56·38	+6·99	11·08	−0·34
76·22	55·22	+6·47	11·19	−0·65
77·26	53·38	+6·46	10·95	−0·68
78·25	51·70	+6·22	10·76	−0·79
79·20	50·13	+5·84	10·55	−0·89
81·26	45·3	+5·9	10·0	−1·2

I have therefore collected the whole of the observations that I could find printed, being materially assisted by the series that Messrs. Gledhill, Flammarion, and Dunér had previously formed, and endeavoured to determine the ellipse that best represented the observations; that is to say, I have treated *Sirius* as an ordinary revolving star, in which case all the sinuosities of the observed path of the larger star can be neglected, since the centre of gravity of the entire system would move in a straight line, or apparently on the arc of a great circle. By the aid of Dr. Auwers' Ephemeris, corrected by the additional formula given by Dr. Dunér in 1874, a normal position-angle for the

* *Comptes Rendus*, August 13, 1877.

beginning of each year was formed. It appeared from the more recent observations that Dr. Dunér's formula, viz.

$$dP = -5^{\circ}0 - 0^{\circ}48\,(t-1869{\cdot}0) + 0^{\circ}03\,(t-1869{\cdot}0)^2,$$

might have been, with advantage, slightly corrected, by increasing the positive term depending upon the square of the interval from 1869, but as the observations were always made near the beginning of the year, and as the angular motion was small, it was not feared that any source of inaccuracy would be introduced by using the formula as given by that astronomer. These position-angles, corrected by the formula

$$-0^{\circ}006\,(t-1870{\cdot}0)$$

to remove the effect of precession, are set down in the second column of Table II.

To ascertain the probable error of these normal position-angles, it was necessary to assign some definite weight to the observations given by various observers. Not being in possession in every case of the full particulars and circumstances under which the observations were made, these weights have been assigned more or less arbitrarily, and perhaps even with fickleness. It was, to avoid this objection, at first intended to use only the results obtained at the Washington Observatory, but as this would have unhappily curtailed the period, already too short, I have been obliged to admit all. I believe that the weight has generally been over-estimated, and that the probable errors given in the third column of Table II. are consequently too small.

To determine the normal distance, I have proceeded somewhat differently. The deviation of the observed distance from the predicted had amounted in 1880 to quite a second of arc (as shown in Table I.), while in 1870 the agreement was nearly identical. The annual motion, which, therefore, in the actual orbit differed materially from that in the theoretical, was derived by means of an interpolating curve drawn among the observed distances laid down to scale. The resulting values of the normal distance with their probable errors, open to the same objection on the score of weight as in the case of the position-angles, are set down in the remaining columns of the table. In a few instances in either element the number of observations was not sufficient to determine the probable error. In the year 1881, the place given rests upon three unpublished observations made in this Observatory, and which Prof. Pritchard permits me to use for the purposes of this investigation, combined with those made at Washington, and given in the *Monthly Notices* of June last.

Table II.

Date.	Normal Position Angle. 1870·0.	Probable Error.	Normal Distance.	Probable Error.
	°	°	″	″
1862·0	85·13	0·09	10·18	0·042
63·0	80·97	0·41	9·64	0·117
64·0	79·76	0·59	10·35	0·157
65·0	76·52	0·25	10·18	0·227
66·0	75·61	0·38	10·49	0·347
67·0	73·73	0·52	10·36	0·398
68·0	71·01	0·36	11·17	0·078
69·0	73·80	0·88	11·22	0·012
70·0	65·48		12·06	
71·0	63·69	1·18	11·31	0·298
72·0	62·72	0·67	11·46	0·034
73·0	62·23	1·50	10·91	0·164
74·0	59·58	1·21	11·36	0·071
75·0	56·94	0·15	11·27	0·144
76·0	55·66		11·22	
77·0	53·45	0·13	11·13	0·069
78·0	52·25	0·20	10·69	0·197
79·0	49·62	0·30	10·62	0·002
80·0	48·52		10·28	
81·0	45·2		9·99	

As it is evident that no regular curve will pass through all the points determined in Table II., these were further submitted to an interpolating curve, so as derive, if possible, a series of points, which, while not differing greatly from the normal positions, might offer better prospects of successful treatment. The position-angles and distances, read from this curve, are called the "adopted position-angles" in Table III., and are the places on which all the subsequent computations have been based.

The differences from the normal positions are recorded under the headings ΔP and Δs; and it will be seen that in each coordinate, there are about eleven changes of sign out of a possible nineteen, so that the drawn curve runs sufficiently well among the observed places. The probable error has, of course, been somewhat increased, but this is immaterial, and is here given merely as a standard whereby the discrepancies that will be derived

from the comparison of places computed from various elements, with the adopted coordinates, may be compared.

TABLE III.

Date.	Adopted Position Angle.	ΔP	Prob. Error of Adopted Pos. Ang.	Adopted Distance.	Δs	Probable Error of Adopted Distance.
1862·0	85°·3	+0°·17	0°·10	9″·78	−0′·40	0″·197
63·0	82·1	+1·13	0·59	10·02	+0·38	·204
64·0	79·6	−0·16	0·71	10·25	−0·10	·653
65·0	77·2	+0·68	0·33	10·45	+0·27	·238
66·0	74·9	−0·71	0·43	10·64	+0·15	·118
67·0	72.9	−0·83	0·55	10·81	+0·45	·507
68·0	70·8	−0·21	0 49	10·97	−0·20	·119
69·0	68·9	[−4·90]	[2·56]	11·11	−0·11	·058
70·0	67·0	+1·52		11·23	−0·83	
71·0	65·1	+1·41	1·36	11·33	+0·02	·298
72·0	63·2	+0·48	0·80	11·39	−0·07	·061
73·0	61·3	−0·93	1·65	11·40	+0·49	·286
74·0	59·3	−0·28	1·22	11·38	+0·02	·072
75·0	57·4	+0·46	0·17	11·30	+0·03	·146
76·0	55·5	−0·16		11·18	−0·04	
77·0	53·6	+0·15	0·14	11·04	−0·09	·078
78·0	51·7	−0·55	0·45	10·89	+0·20	·278
79·0	49·7	+0·08	0·32	10·71	+0·09	·086
80·0	47·7	−0·82		10·48	+0·20	
81·0	45·4	+0·2		10·06	+0·07	

It will be observed that the distances indicate a tolerably well defined maximum about the year 1873, and therefore there should be a minimum as strongly pronounced, in the annual motion about the same year. This cannot be detected in the observations, and hence it is evident that two distinct orbits can be derived, according as we assume the distances as given by observation or as computed from the formula

$$r^2 \frac{d\theta}{dt} = \kappa.$$

In presence of this difficulty, I have first assumed the period to be that required by the discussion of the proper motions of *Sirius*—namely, 50 years—and with that period have derived the following orbit:—

Elements A.

Time of Periastron Passage	$T = 1891{\cdot}51$
Excentricity	$e = 0{\cdot}5046$
Angle between Axis Major and Node Line ...	$\lambda = 219^\circ\ 30'$
Inclination	$i = 55\ 0$
Node	$\Omega = 48\ 46$
Period	50 years
Mean Distance	$a = 8''{\cdot}32$

As will be seen from Table IV., the agreement between the adopted position-angles and those computed from these elements is satisfactory. That of the distances is less so. Omitting the error for 1881, for which date the distance is insufficiently determined, the sum of the squares of the discordances expressed in hundredths of seconds is 6349, while the sum of the squares of the probable errors for the same dates, given in Table III., and expressed in the same unit, is 6117. It will be interesting, therefore, to try if the distances be better represented by a different period, and also whether the limits can be fixed between which the time of revolution must lie. With this view, the period was successively reduced to 47 and to 44 years, and by the method of varying each element a new orbit was found.

	Elements B.	Elements C.
T	1891·62	1889·44
e	0·5219	0·5908
λ	$217^\circ\ 13'$	$223^\circ\ 21'$
i	56 47	58 37
Ω	48 12	45 27
a	$8''{\cdot}50$	$8''{\cdot}53$
Period	47 years	44 years

In the following table is given the comparison between the adopted places and those computed from the elements successively for each alternate year, more frequent comparisons not having been thought necessary:—

TABLE IV.

Date.	Adopted Position Angle.	C−O Elements A.	C−O Elements B.	C−O Elements C.	Adopted Distance	C−O Elements A.	C−O Elements B.	C−O Elements C.
1862·0	85°·3	−0°·2	0°·0	0°·0	9″·78	−0″·46	−0″·72	−0″·84
63·0	82·1	+0·3	+1·2	+0·2	10·02	−0·35	−0·54	−0·63
65·0	77·2	+0·2	+0·7	−0·2	10·45	−0·16	−0·23	−0·25
67·0	72·9	+0·1	+0·4	−0·3	10·81	−0·02	+0·03	+0·06
69·0	68·9	0·0	+0·1	−0·4	11·11	+0·04	+0·21	+0·26
71·0	65·1	0·0	0·0	−0·3	11·33	+0·01	+0·29	+0·37
73·0	61·3	−0·1	−0·1	−0·1	11·40	−0·03	+0·31	+0·42
75·0	57·4	0·0	−0·2	+0·2	11·30	−0·11	+0·30	+0·41
77·0	53·6	−0·3	−0·2	+0·2	11·04	−0·24	+0·20	+0·30
79·0	49·7	−0·9	0·0	−0·6	10·71	−0·45	+0·12	−0·06
81·0	45·4	−0·8	−0·5	−0·6	10·06	−0·62	−0·12	−0·47

It will be observed that the position-angle is sufficiently well represented in all three orbits, but that the distance disagrees by a greater amount than can be fairly attributed to the errors of the adopted places. The sums of the squares of the errors, which, on the assumption of a period of 50 years, amounted to 6349, rose, as the period was decreased to 47 and 44 years, to 12181 and 17312 respectively; from which it may be inferred that the period is not much less than 50 years, and certainly more than 47.

I have not been so fortunate in fixing a period which the time of revolution cannot exceed, for I find that if the period be greatly increased an entirely different set of elements will represent the observed places with fair accordance. For instance, if we assume the elements thus

$$T = 1857{\cdot}27$$
$$e = 0{\cdot}9011$$
$$\lambda = 63^\circ\ 50'$$
$$i = 75\ 58$$
$$\Omega = 93\ 35$$
$$a = 54''{\cdot}08$$

Period 442 years.

The adopted places are represented within the following errors:—

Date.	Pos. Angle. C−O	Distance. C−O
	°	″
1862·0	−0·4	−0·17
65·0	+0·5	+0·23
68·0	+0·6	+0·08
71·0	+0·2	−0·19
74·0	−0·1	−0·27
77·0	−0·5	+0·01
80·0	−0·8	+0·53

Doubtless these errors could have been reduced by continued computations, but no useful purpose would be served by prolonging the discussion in this direction. This approximate result is given here to exhibit a remarkable instance of the difficulty of determining the orbit of a double star when the arc of observation is not sufficiently extended. If, as in most instances, there had been no reason from independent sources to suspect a short period, these last elements would have been worthy of favourable consideration. There are, however, two circumstances which suggest their rejection. First, the elder Herschel or W. Struve never observed the duplicity of the bright star, and at the commencement of the century the position of the small star would have been

$$\theta = 283^\circ; \qquad s = 5''\cdot7$$

if these latter elements are approximately correct. At such a distance from the primary it is difficult to understand how, not only Herschel, but so many astronomers failed to observe it. Secondly, with a parallax of $0''\cdot193$, which is that assigned to *Sirius* by Dr. Gyldén, the mass of the system would be more than one hundred times the mass of the Sun. This, too, is very noticeable in relation to the masses of other stars which have been hitherto approximately determined. The argument drawn from considerations of annual parallax is, however, inconclusive, because a not impossible increase in the value deduced by Dr. Gyldén would reduce the mass of the system within probable limits. And, moreover, if the parallax furnishes a reason for the rejection of the longer period, it offers as great a difficulty to the acceptance of the shorter. For, using the same value of the parallax and adopting the mean distance given in Elements A, the resulting value of the mass of the *Sirius* system is some 32 times that of the Sun. But Dr. Auwers has shown that the perturbing body has a mass of about one-half that of *Sirius*. This makes the mass of the companion about ten times that of the Sun, whence we should expect a more conspicuous object than the satellite presents.

The conclusion to which we are driven is that, notwithstanding that observations of this interesting system have been made

for nearly twenty-years, or during two-fifths of the entire period of the motion of *Sirius*, they do no more than render the connection extremely probable, and will as yet not decide definitely whether this companion be the perturbing body or another member of a more complex system. But as the Elements A differ from those given by Dr. Auwers, it is desirable to trace what effect these alterations of the elements have in explaining the irregularities of the observed Right Ascension and Declination of *Sirius*. This is a question to which I hope to return at a future opportunity.

Oxford University Observatory:
1881, *December* 8.

Note on Messrs. Campbell and Neison's Paper on the Parallactic Inequality, in the Supplementary Number of the Monthly Notices. By E. J. Stone, M.A., F.R.S.

I. Messrs. Campbell and Neison appear willing to leave the discussion of their treatment of the question of the irradiational enlargement of the Moon as it stands. I gladly accept their decision on this point: I am quite content to leave the discussion as it stands.

II. But I cannot allow to pass unchallenged the statements which Messrs. Campbell and Neison have been pleased to make respecting the correction which the Meridian Observations of the Moon, 1851–1858, as given in the Greenwich volume, 1859, require for the error of adopted semi-diameter.

The adopted semi-diameter is clearly stated by Sir G. B. Airy to be Adams's value. The following are the corrections which I have deduced from a discussion of the observed durations 1851–1858:—

Year.	No. of Observations.	Adams's value too large by
		″
1851	7	+0·43
1852	3	−0·71
1853	4	−0·97
1854	3	+1·17
1855	3	+0·94
1856	3	+1·77
1857	3	+0·76
1858	5	+1·02

The deduction of these values is troublesome, and requires some care, on account of the many changes which were made in the adopted quantities of the *Nautical Almanac*, 1851–1855. But, although the results for 1852 and 1853 are rather discordant, I

can find nothing wrong in the work, and I believe they have been fairly deduced from the observations. If all the observations are given equal weight, the result is that Adams's semi-diameter is too large by 0″·51; whilst from all the observations made with the Transit-Circle 1851–1861 it would appear that Adams's semi-diameter is too large by 0″·63 to represent the observed durations.

I must leave Messrs. Campbell and Neison to explain how they justify the assertion "in all our results we have employed the observed value," whilst they assume in their work that Adams's semi-diameter is too *small* by 0″·18 to represent these observations. I need hardly point out that, unless Messrs. Campbell and Neison can prove that Adams's semi-diameter is too small by about 0″·18 to represent these observations 1851–1858, the very remarkable agreement between the values of the coefficient of the parallactic inequality, to which they appealed in the *Monthly Notices* for March 1881 as proving the existence of the forty-five-year period inequality in the expression for the Moon's longitude, no longer exists.

III. I next pass to the supposed effect of Mr. Ellis's personal error in observing the limbs of the Moon on the coefficient of the parallactic inequality 1843–1851. I give in the following table the actual differences of the longitudes of the centre as deduced from observations of each limb. This shows at once the effect of the error of the assumed diameter. It is clear that Messrs. Campbell and Neison are mistaken when they attribute the large value of the parallactic inequality given by the observations 1843–1851 to personal error in Mr. Ellis's observations of limbs.

1843–1851.

Date.	Longitude of Centre. $1\,\mathbb{D} - 2\,\mathbb{D}$.	Observer.
1843, May 13	−0·25	E.
Aug. 9	+1·68	M.
Dec. 6	+0·96	H.
1844, May 2	−2·43	R.
June 29	+1·10	E.
Sept. 26	+3·85	R.
1845, June 19	+2·67	H.
Sept. 15	−2·62	E.
1847, Mar. 31	+1·78	R.
May 29	+1·94	R.
July 27	+2·66	H.
1848, Feb. 18	0·00	R.
Mar. 19	+5·91	H.
April 17	+1·67	M.

Date.	Longitude of Centre.		Observer.
	1 ☽ − 2 ☽.		
1848, Sept. 12	+1·93		H.
1849, Mar. 8	+0·95		H.
Aug. 3	+6·69		M.
1850, Mar. 27	+0·28		R.
Aug. 22	+0·42		R.
Oct. 20	+3·61		D.
1851, Feb. 15	+0·41		H.
May 14		−0·30	M.
June 13	+0·98		J. W. B.
July 12	+0·83		E.
Aug. 11	+1·25		W. E.
Sept. 9		−0·39	D.
Dec. 8	+2·51		R.

The observers are Messrs. Main, Henry, Richardson, Ellis, Dunkin, Breen, and William Ellis.

The error of the adopted semi-diameter is $-0''\cdot705$ from the whole of the observations, and is $+0''\cdot235$ from Mr. Ellis's observations alone.

The value of the coefficient of the parallactic inequality deduced directly from the observations with the adopted semi-diameter is

$$124''\cdot52.$$

The value which would be given with the mean correction is, very approximately,

$$125''\cdot26.$$

But, if we applied a correction deduced from Mr. Ellis's observations alone, we should still find a value

$$124''\cdot39.$$

The value deduced by Messrs. Campbell and Neison, with a correction for the supposed forty-five-year period inequality, was

$$122''\cdot75.$$

The excess of the observed value, even on the extreme supposition that Mr. Ellis's diameter should be alone used for the reduction of all the observations, is still $1''\cdot6$ over the computed value. How, then, can Messrs. Campbell and Neison assert that "with this correction the discordance vanishes"?

Note on the Variable Star D.M. +1° No. 3408. By Professor C. Pritchard.

On the receipt, Dec. 5, of the Dun Echt Circular, No. 41, relative to the variability of the star D.M. +1° No. 3408, after the manner of the Algol type, I instructed Mr. Plummer to make comparisons of its brightness by the photometric means which I described in the *Monthly Notices* of last month.

The comparison stars taken were D.M. +2° No. 3283 (α) and +1° No. 3411 (β). The variable star is denoted by V.

	h	m			
Dec. 5,	4	57	G.M.T.	... ...	V−α = ·31 magnitude
7,	5	9		... ...	V−α = ·30 ,,

These results were further confirmed by comparison of (α) with (β), and each with V, thus—

	h	m			
Dec. 7,	5	28	G.M.T.	... ...	β−V = ·34 magnitude
7,	5	14		... ...	β−α = ·54 ,,

These last two comparisons would give V−α = ·20 magnitude, implying a discordance of only one-tenth of a magnitude among these insufficient observations, made, as will be explained, under somewhat unfortunate circumstances. In these results, regarding the brightness of the variable at the above epochs, there is little or nothing noteworthy, inasmuch as the variation of light is said to be of the Algol type, and neither of the times of observation is that of a minimum as recorded by Mr. Sawyer. Assuming with Argelander that $\alpha = 6{\cdot}5$ mag., V = 6·8 magnitude. This is altogether at variance with Argelander's estimate 5·5; an estimate certainly not by any means borne out by the eye on Dec. 5 and Dec. 7, and which eye-observation agrees substantially with the wedge-reading. Neither do these photometric observations agree with Mr. Sawyer's estimate of variation from the 6th to 6·7 magnitude; but we are left in doubt as to Mr. Sawyer's standard of magnitude.

Unfortunately, the Sun is even now far too close to the star to admit of a very reliable determination of its brightness, or varying brightness, and this increasing proximity will effectually prevent further comparison for some time to come. The very singular discordances recorded in the estimates of its magnitude, varying from 5·5 to 8·0 magnitude, by astronomers so able as Schjellerup, Lamont ,and Argelander, will render this star an object of interest and inquiry a few months hence, when the Sun will have quitted its vicinity. The above observations are to

be regarded simply as a record for comparison with other observers, who may also have already made similar measures.

It may interest some members of the Society who are skilled in astrometry, to be informed that the photometric measures of the relative brightness of all the stars in *Lyra* contained in Dr. Heis' Catalogue, 75 in number, together with two others which ought have been inserted on account of their brilliancy, have now been completed at this Observatory by three sets of five readings on two nights. The consistency of the results is encouraging, and leaves no doubt in my mind of the facility and accuracy of the method devised. I have accordingly arranged for the systematic prosecution of these observations for all the stars visible *nudis oculis* from the Pole to 5° South Declination. This will embrace all the stars recorded in Dr. Heis' Catalogue within the limits prescribed.

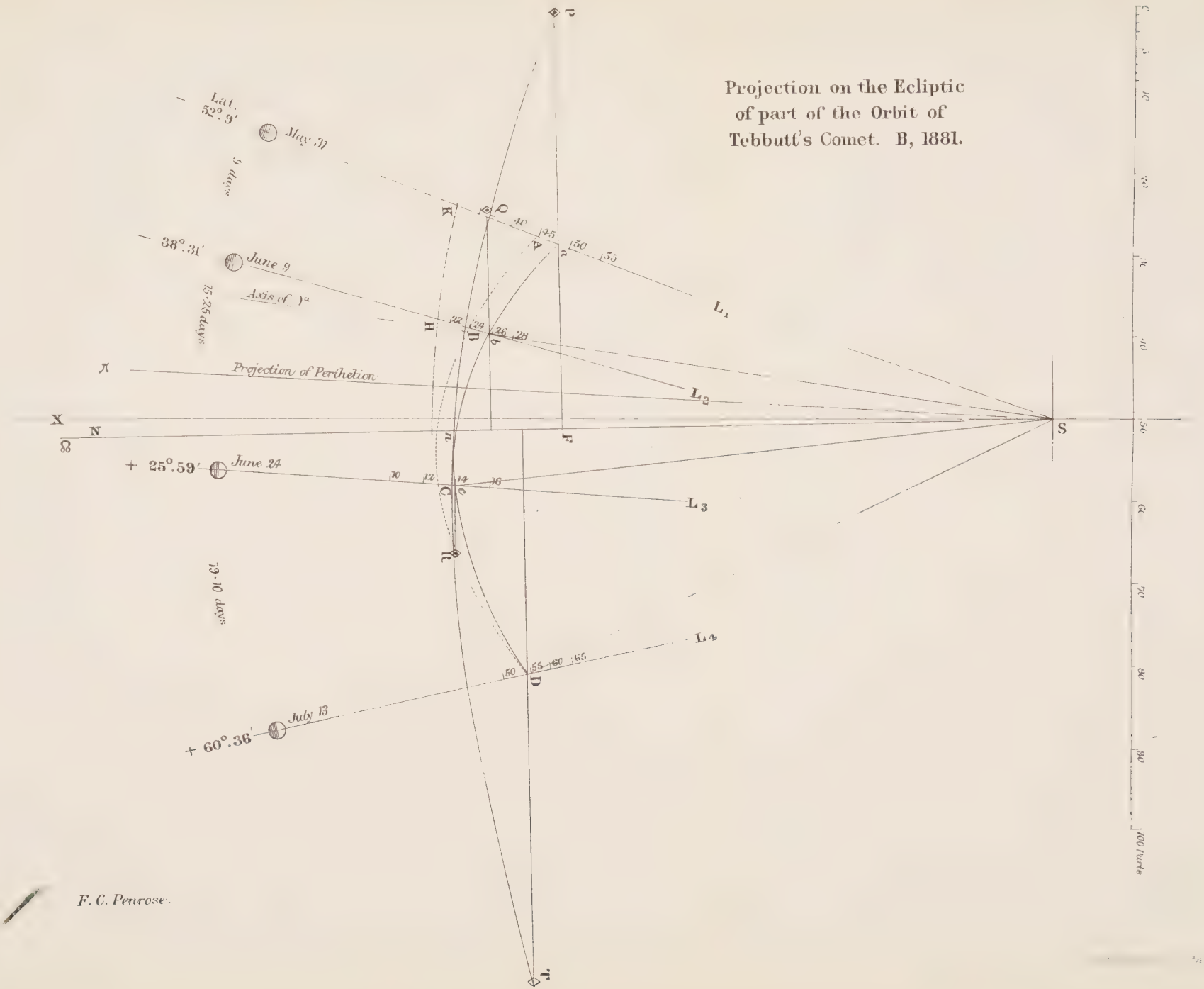
Oxford University Observatory:
1881, *December* 8.

On a Method for Finding the Elements of the Orbit of a Comet by a Graphical Process. By F. C. Penrose, Esq.

It may be interesting to those who are not familiar with the analytical methods of working out the orbit of a Comet to examine a process of graphical construction which will, in many instances at any rate, suffice to give very rapidly a good approximate solution by means which demand only conversance with the methods of practical geometry and sufficient spherical trigonometry to reduce Declination and Right Ascension into latitude and longitude.

I was first led to try if by graphical operations I could get a general idea of the motion of Tebbutt's Comet (1881, *b*), and finding that I succeeded to a greater extent than I had anticipated, I proceeded to attack Schäberle's Comet *c*, with some diffidence, indeed, as it seemed to be giving a good deal of trouble to the computers, and obtained a result which I found compared favourably with the published elements. I venture, therefore, to think that these attempts may be worth laying before the Society.

I should premise that I do not think that much reliance could be placed on a graphical solution of the orbit of a Comet from a few days' observation only, unless it should happen to be near the Earth or to the perihelion; and also that four places are much better than three, although it would be of course theoretically possible to find the elements from three. I will begin with the case of Tebbutt's Comet (Comet *b*, 1881).


Projection on the Ecliptic
of part of the Orbit of
Tebbutt's Comet. B, 1881.
Lat. 52°.9'
May 31
9 days
38°.31'
June 9
Axis of
25·25 days
Projection of Perihelion
+ 25°.59'
June 24
19·10 days
+ 60°.36'
July 13
L1
L2
L3
L4
S
T
N
X
K
H
A
B
C
D
E
F
G
Q
R
40
45
50
55
22
24
26
28
20
12
14
16
50
55
60
65
700 miles

F. C. Penrose.

I take four observations reduced to the ecliptic.

	G.M.T.	Longitude.	Latitude.
Cape of Good Hope	May 31·21	69° 38′ 6″	−52° 9′
	June 9·18	74 16 36	−38 31
Greenwich ...	June 24·48	86 19 6	+25 59
	July 13·58	102 37 33	+60 36*

The apparent places are taken, as minute accuracy is not attempted.

The plane of the paper represents the ecliptic, S being the Sun's centre. The positions of the centre of the Earth are calculated for the times given above, and laid down on the diagram at the points entered, with the dates (May 31, June 9, &c.), and from these are laid down the longitude lines (L_1, L_2, &c.). The straight line SX is drawn at 90 degrees from that of the equinoxes. In the scale used six inches represents the Sun's mean distance. For working the solution it is desirable that this scale, divided into 100 parts, should be drawn upon the edge of slip of stout paper or card. The slide rule may also be used with advantage to facilitate the necessary calculations.

The first thing to be done is to obtain a rough approximation to the place of the projection of the Comet where it passes the different longitude lines (L_1 L_2, &c.). To get this it is convenient first to form a short table of numbers proportionate to the time intervals between the dates of the observations, which are respectively about 9, 15·25 and 19·1 days.

9	15·25	19·1
10·6	18·00	22·5
11·2	19·00	23·7
11·8	20·00	25·0
12·3	21·00	26 2
12·9	22·00	27·5

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* The observations are these, namely :—

(1) *Cape of Good Hope.*

Cape Mean Time.	R.A.	Declination.
h m s	° ′ ″	° ′ ″
May 31, 6 10 24	75 46 25	−29 42 19
June 9, 5 31 18	77 16 24	−15 44 53

(2) *Greenwich Observations.*

	G.M.T.		N.P.D.
		h m s	° ′ ″
June 24,	Transit *sub polo*	5 39 39	40 36 35
	h m s		
July 13,	13 52 0	...	...

Comet observed in Transit-Circle together with 37 Camelopardi.

δ R.A. (Comet − Star) + 56^s·85

δ N.P.D. (Comet − Star) − 0″·4

The mere inspection of the lines L_1 L_2, &c., is sufficient to show that the Comet was passing with a direct course between the Earth and the Sun. Lay the edge of the card or paper scale across these lines and bend it round in such a curve as will cut off upon the scale numbers giving a tolerable approximation to one of the sets in the table given above. For instance, with 20 on the middle arc CB we shall find the other two places fairly represented by A on L_1 and D on L_4; but it will be seen that BA exceeds 11·8, CB being 20. But as the areas about S must be proportional to the times in the projection on the ecliptic as well as in the orbit, it follows that if BA is nearer to S, or lies more obliquely with respect to S than CB, it may exceed the number parallel to 20 on the table; whereas, for the same reason, no such course as the dotted line HK could be chosen.

In this first operation it is almost impossible that the numbers should come out exactly, and it is not worth while to occupy much time upon it. It is, however, important to get fairly proportionate numbers on each side of the middle arc, and in this case the places ABCD, which appear to give the measurements from the scale of 13·5, 20 and 26, will answer for a first start.

The areas of the segments about S must be now examined more carefully. If the points had been correctly chosen

$$\frac{\text{area SBC}}{15{\cdot}3} = \frac{\text{area SCD}}{19{\cdot}1} = \frac{\text{area SAB}}{9};$$

but on comparison we shall find both SCD and SAB to be too large. As respects SCD an obvious improvement will arise from approaching nearer to S, because the lines L_3 and L_4 converge rapidly, and when the points A and C have been advanced to a and c the areas S a c and S c D will have a better relation to one another. Also if on L_1 A be shifted to a the areas S a b and S b c will be found to be more nearly adjusted, and the proposed curve now becomes a b c D.

Thus far we have only considered the longitudes and the areas resulting from them; but the latitudes should now be examined. And it will be convenient first to mark off upon each of the longitude lines, near the places where the curve crosses them, the points where the height above or below the ecliptic has such values as 40, 45, 50, 55 on L_1, 22, 24, 26, 28 on L_2, &c.—these points being given by the cotangents of the latitude.

We shall see that the amended curve cuts L_1 where this reading is about 47·5, and L_4 at about 56, whilst the horizontal distance between the points is about 52. Half the sum of these heights divided by half the horizontal distance should give a value for the tangent of the angle of inclination,

$$\frac{51{\cdot}75}{26} = \tan 63^\circ\ 20'.$$

From L_2 and L_3, by the same method, we get

$$\frac{19{\cdot}9}{9{\cdot}3} = \tan 64^\circ\ 55'.$$

Observing this difference, it would seem that b and c have been carried rather too near S, because the latitudes at B and C would have given for the value

$$\frac{24+13}{19},$$

giving 62° 50′; but it is undesirable to complicate and confuse the diagram, on so small a scale as is necessary to accompany this paper, with more lines than are shown upon it, and it will suffice for the present purpose to combine the two readings, viz.—

$$\frac{51{\cdot}75+19{\cdot}9}{26+9{\cdot}3} \text{ or } \frac{71{\cdot}65}{35{\cdot}3} = \tan 63^\circ\ 50'.$$

To obtain the line of the nodes we must measure off from b or c a distance corresponding to the cotangent of the angle of inclination, and thus obtain $cn=6{\cdot}82$, and we can now draw SN for the line of the nodes, which makes an angle of 1° with SX, and which, reckoned in longitude, will be 271°.

The next process is to develop the projection so as to get the true figure of the orbit by drawing through the points $a\ b\ c$ D perpendiculars to SN, and cutting off points upon these lines as determined by the secant of the angle of inclination—viz. such as $PF=Fa\times\sec 63^\circ\ 50'$.

It will now be convenient to prepare and transfer to tracing paper the arc of a parabola of which the focal distance at the vertex may be taken as Sn.

Should it, however, not be obvious that Sn is the proper distance, the way to proceed would be this. Take the points Q and R, and with twice the distance SQ or SR draw arcs of circles, which will intersect in some point beyond S. From this point draw a straight line through S back again to the figure, and it will give the approximate place for the vertex of the parabola, and also determine the parameter. With these parabolic elements construct, as before proposed, on tracing paper an arc of sufficient length to reach from P to T. Then, keeping the focus upon S, turn the tracing about until it makes the best possible coincidence with the points P, Q, R, T, and then it will point out the perihelion, the time of the passage in the developed orbit plane, and, by projection, its longitude on the ecliptic; and it will also give an improved value of the distance.

Following the indications of this small scale figure, the results would be

T	... ...	June 14·70
π	*i.e.* Longitude of Perihelion	266·37
q	Distance	0·73 or [9·8633]
Ω	... ...	271° 0′
i	... ...	63 50

I have not tried this Comet on a scale adequate to obtain the best possible graphical approximation, but there is no doubt that the elements above given could be brought nearer the results of calculation which are given below, according to Mr. Hind's parabolic elements, especially as to the date of the perihelion.

T	... ...	June 16·457
π	... ...	265° 15′ 44″
q	... ...	[9·8657]
Ω	... ...	270° 57′ 46″
i	... .	63 28 46

The other example which I give is Schäberle's Comet, *c*, 1881. Observations on five days have been considered

	G.M.T.	Longitude.	Latitude.
Oxford Radcliffe Observatory	July 31·41	95° 19′ 56″	+22° 34′ 17″
	Aug. 4·42	99 2 24	25 8 9
	10·44	108 5 14	29 49 0
	19·53	137 33 22	36 7 0
Marseilles	Sept. 2·33	199 18 0	17 23 0*

As in the former case, SX is drawn at 90° from the line of the equinoxes, and the same scale is used as in the former diagram.

The first process is to lay down the longitude lines L_1, L_2, L_3, &c., which show that we have now the case of a retrograde orbit, and that the Comet is passing between the Sun and the Earth, and, as the latitudes are all positive, it is, within the above limits, to the north of the ecliptic.

The intervals between the first four observations are respectively 4·01, 6·02, and 9·09 days. From these we determine the following ratios, viz.:—

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* The observations which I have followed are—

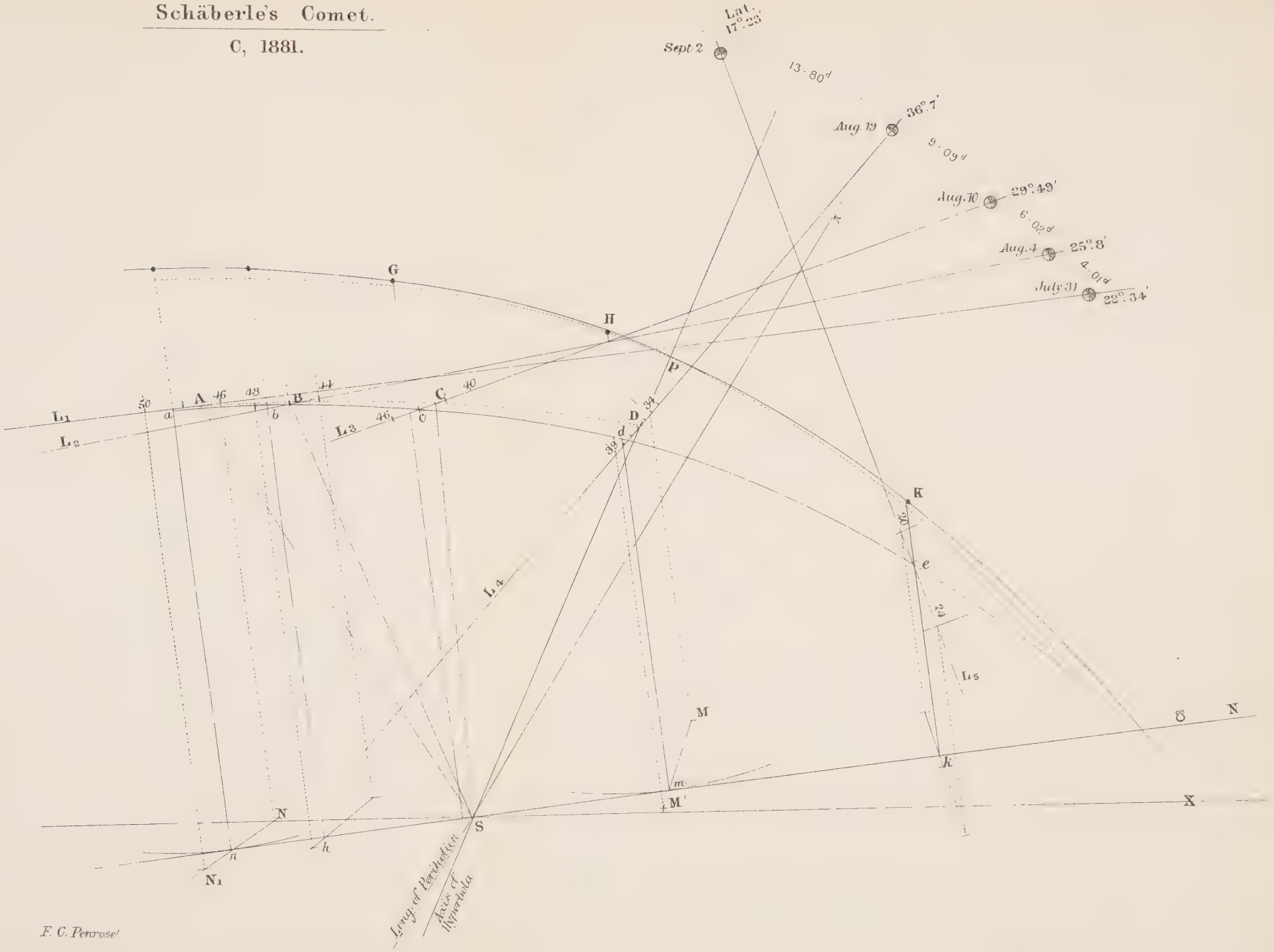
Oxford Radcliffe Observatory.

Passage of Comet *sub polo*.

G.M.T.	Observed R.A.	Observed N.P.D. uncorrected for parallax.
	h m s	h m s	° ′ ″
July 31,	9 57 43·2	6 28 21·6	44 3 46·1
Aug. 4,	10 32 53·5	6 49 17·7	41 47 13·1
10,	10 29 50·1	7 42 55·6	38 21 32
19,	12 35 2·4	10 23 57·4	40 44 51
Marseilles.			
Sept. 2,	8 0 0	13 37 27	81 31 0

Schäberle's Comet.

C, 1881.



4·01	6·02	9·09
8	12	18·1
10	15	22·6
12	18	27·2
14	21	31·7

If we bend the scale across the longitude lines for a first approximation, we shall find that 12, 18, and 27·2 would agree with the dotted line ABCD pretty nearly; but on drawing a curve through these points and joining them with S, we should find the area ASB to be too small in comparison with BSC, whilst CSD is too large, showing that the projection of the orbit must lie more nearly in the direction *a b c d*. Before that line, however, can be adopted, the latitudes should be examined, and the scales of height above the ecliptic marked upon L_1, L_2, &c.

The line of the nodes can now be drawn approximately. It must pass through S and cannot be far from SN, which direction will follow from the condition that there must be an invariable proportion between the heights above the ecliptic and the length of all such base lines as *a n*, *b h*, &c., this ratio being the tangent of the angle of inclination of the orbit, and this angle will have to be determined simultaneously with the direction of line of the nodes. In getting the angle of inclination primary attention should, in this particular case, be paid to the point C, because a moderate variation in the direction of SN will affect the length of the perpendicular drawn from C in the direction CS very little, and a first trial appears to give for the tangent of the angle of inclination $\frac{43}{51{\cdot}7}$, or 39° 30′; but as this angle will not also satisfy the ratios obtained from the other points, we shall see that C must be moved onwards, as to *c*, where the value of the tangent becomes $\frac{44{\cdot}6}{51{\cdot}7}$, or 41°; and using this value for the inclination, we may proceed to obtain a more exact determination of the line of the nodes.

Looking first at the longitude line L_1, and observing that the approximate curve cuts it near *a*, where the value on the scale formed on L_1 is about 48·5, draw *a n* proportional to the cotangent of 41°. Then from another of the longitude lines (say L_4) draw *d m*, bearing the same ratio to 38, which is the height shown on the scale formed on L_4. These lines *a n* and *d m* may be used as radii for striking arcs of circles, as shown in the diagram, to guide the direction of SN. If the work has been correctly done, *n*, *s* and *m* will lie in the same straight line: if not, draw SN parallel to the line touching the two circular arcs, and take this as the corrected line of the nodes.

On each of the longitude lines, and on each side of the points

a, b, c, &c.—viz. from 48 and 50 on L_1, from 44 and 48 on L_2, and so on with regard to the other points—drop perpendiculars on SN proportional to the cotangents of the inclination, and join their extremities as NN_1, MM_1, &c. The line of the nodes will intersect these base lines at n h m k; and from the last-named points draw verticals to meet the longitude lines. These will give the corrected places for the projection of the orbit. The areas may now be re-examined, but if the work has been correctly done they will probably be found to satisfy the eye. The method above given has been somewhat more complicated than in the case of Comet b, but in that case there were observations available on each side of the ecliptic.

We may now proceed to develop the orbit from the projection, as shown by the dotted lines, by multiplying the verticals already obtained by the secant of the angle of inclination.

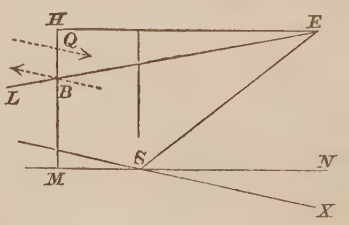
It is quite obvious that the perihelion distance occurs not far from the point H of the developed orbit.

If a circle be drawn through the three points G, H, and K it will show that the nearest point to S will be at or near P. With this distance for its focal length, construct a parabola as in the former case; and after twisting the figure about in the manner already described, we shall obtain the position of the perihelion P and a further correction of the distance. Correct, if necessary, the parabola to the distance so determined, and apply it again to the developed orbit, and it will be found that in this case the points, except those close to the perihelion, will lie in regular sequence outside the parabolic arc, demonstrating that this Comet's orbit must be distinctly hyperbolic. The axis of the curve passes of course through the perihelion, and if projected on to the ecliptic we obtain its longitude.

The elements obtained from this figure are

T	...	...	...	August 22·3
π	...	...	...	$147^\circ\ 30'\ 0''$
q	...	...		0·627 or [9·7973]
Ω	...	...	...	$95^\circ\ 55'\ 0''$
i	...	...	...	$41^\circ\ 0\ 0$

In the case of this Comet I have also endeavoured by means of a larger scale, and by the use of trigonometrical formulæ instead of some of the graphic work to obtain a still closer appoximation.



To explain this method of working, I take one of the points of the former diagram, for instance B. Let the angle

$$\text{ESN} = \alpha$$
$$\text{SE} = r$$
$$\text{EB} = \rho$$
$$\text{HB} = x$$

Let EH be parallel to SN, and let the angle $\text{HEB} = \gamma$, and let l be the latitude of the Comet as seen from E.

It then follows that

$$\rho \tan l = \text{MB} \tan i$$
$$\text{MB} = r \sin \alpha - x, \text{ and } \rho = x \operatorname{cosec} \gamma$$
$$\therefore (r \sin \alpha - x) \tan i = x \operatorname{cosec} \gamma \tan l$$
$$\frac{r \sin \alpha}{x} - 1 = \operatorname{cosec} \gamma \tan l \cot i$$
$$\text{and } x = \frac{r \sin \alpha}{\operatorname{cosec} \gamma \cot i \tan l + 1}.$$

From this it is easy to obtain MB, MH, and SM by calculation, and it will be found more accurate to lay the points which are wanted down by scale than to obtain them graphically from the intersections of lines. If we want the point Q of the developed orbit, it is got from

$$\text{MB} \sec i.$$

When the places in the orbit have been determined in this manner and laid down on the drawing, let the approximating conic section be applied, and it will be seen where the ordinates and abscissæ require modification: this must be done by small variations of the inclination and the line of the nodes; but as these elements affect the different places differently, such modifications of each must be chosen as will best suit the general purpose. To estimate the effect of the different variations we might take the differential coefficients with respect to the angle XSN (which angle may be called ω), and also with regard to i, and apply them to each case. But it is very little more trouble to compute the places directly for slightly different values of i and ω, which has been done to form the following table. The observed places have also been corrected for parallax, &c.

			July 19·164	July 31·41	Aug. 19·53	Sept. 2·33	Sept. 21
i	41° 0′	MQ	·7416	·7419	·5836	·3269	·1130
ω	5 40	SM	·6436	·3146	·2405	·5909	·9005
i	41 0	MQ	·7514	·7468	·5827	·3253	·1123
ω	5 56	SM	·6668	·3223	·2433	·5919	·8964
i	41 15	MQ	·7544	·7482	·5801	·3248	·1123
ω	5 56	SM	·6755	·3261	·2414	·5923	·8966

From this table the effect of small changes in i and ω upon the different points can be easily ascertained and a choice made of such changes as will best suit an approximating hyperbola. It may, however, be found necessary to reconsider the conic elements, but the same table can again be used for a final adjustment. Working in this manner I have arrived at the following elements of the orbit, which give a very close agreement at all the places that I have tried, and also give sensibly areas proportional to the times.

T	... ...	August 22·61
π	... ...	146° 44′ 0″
q	... ...	0·629 [9·79865]
Ω	... ...	95° 40′ 0″
i	... ...	40 48 0

And the hyperbola which seems to coincide best with the orbit has for its major axis twelve times the mean distance of the Sun and $e=1{\cdot}052416$.

The elements which are given in the *Astronomische Nachrichten*, No. 2390,

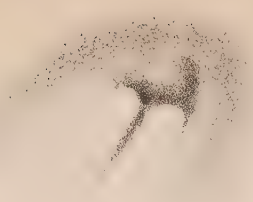
T	... ...	August 22·774
π	... ...	218° 36′ 39″ or 141° 23′ 21″ according to the way it is reckoned.
q	... ...	[9·80018]
Ω	... ...	96° 25′ 48″
i	... ...	139 50 17 or 40° 8′ 43″ according to the way it is reckoned.

Notes on Sketches of Comet b, 1881. By E. B. Knobel, Esq.

June 24.—The head presented a curious unsymmetrical form: an irregular fan, much developed on the following side, and with little extension on the preceding side; emanating from this in the direction of the tail, was a bright ray, as shown in the sketch made that evening. The brightest portion of the fan was that in front, or nearest the Sun. The nucleus was eccentrically situated in the fan, on the preceding side of the bright ray, which certainly did not radiate from the nucleus. An envelope, apparently of irregular form, surrounded the fan, the curve of which was not symmetrical with the head; exterior to this was the parabolic envelope.

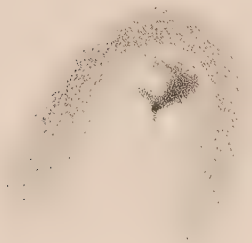
June 29.—The ray seen June 24 had disappeared. The fan was more symmetrical in shape, and the brightest portion was on the preceding side of the nucleus. On this date, at about 11·15, the nucleus passed very close to a 7·4 mag. star, Rad. 1661.

COMET b 1881.



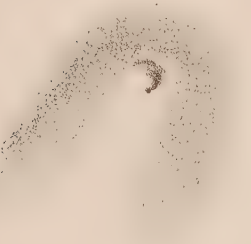
June 24. 11 hrs

June 29. 12 hrs.



June 30. 11 hrs

July 1. 11 hrs



July 3 12 hrs

July 4. 10 hrs.

July 6. 11 hrs

July 11. 10½ hrs.

B Knobel . del.

Spottiswoode & Co Lith. Lond.

A series of measures of distance, taken one after the other as rapidly as possible, gave:—

16″·5
18·9
20·3
20·6

The motion of the Comet was so perceptible and so rapid that accurate measures were impossible. The intervals of time between the measures were not noted.

June 30.—The appearance of the Comet had much changed; the brightest portion of the fan was on the following side. A small ray or brush of light, slightly inclined to the direction of the tail, emanated from the nucleus. There appeared to be a bright space in front and surrounding the fan, and exterior to this a dark envelope.

July 1.—The fan appeared smaller and symmetrical; the nucleus small and stellar.

July 3.—The nucleus was very small. The fan appeared to be reduced to a curious wisp, springing from the nucleus and curved towards the preceding side.

July 4.—The fan is symmetrical and somewhat resembling the appearance on June 30, with the brightest portion on the following side.

July 6.—The fan is symmetrical, the central portion being the brightest, and resembling the curved wisp observed July 3. A slight brush of light was noticed emanating from the nucleus in the direction of the tail.

July 11.—The fan small and reduced to a very similar appearance to that noticed on July 3.

Observations of Mimas, 1881. By A. A. Common, Esq.

I give below some conjunctions of *Mimas* with end of ring, as observed with the three-foot reflector.

	G. M. T. h m	
Nov. 15, at	12 43,	*Mimas* was seen approaching *sf* conjunction, and level with division in ring.
	12 59,	not up.
	13 11·5,	just seen nearly up, and not seen again (though looked for during half an hour), although no difference could be seen in the sky to account for this.
Nov. 17, at	10 20,	*Mimas* not yet up.
	10 22,	steadily seen, not up.
	10 24,	up to *sf* conj.
	10 29,	past.

Nov. 19, at 12 23, near *nf* conj.
12 34, up; difficult to see.
12 39, considered past.
Nov. 20, at 5 55, *Mimas* not yet up to *sf* conjunction.
6 8, thought to be about up.
6 14, considered past.
Nov. 22, at 6 55, *Mimas* and *Enceladus* were nearly in a line drawn perpendicular to the major axis of ring, *Mimas* being nearest the end of ring.
7 4 to 7 6, they were in a line.
7 19, *Enceladus* much the nearest to end of ring.
8 24, *Mimas* not yet up to *nf* conjunction.
8 29, very near, if not up.
8 34, now up.
8 41, past; hard to see.

This satellite has been seen at other times, and is generally visible when clear of ring; but it is a most difficult object in my three-foot telescope under the best conditions, when near the end of ring, even now when it is at some little distance apparently, *Hyperion* being very much easier to observe even at conjunction; it is therefore easily seen any fine night, when the Moon is not too near.

Ealing:
1881, *December* 8.

Note on the Discovery of Comet c 1881 (*Schäberle*).
By W. F. Denning, Esq.

From the heading to Captain Maling's paper in the *Monthly Notices* for November, 1881, p. 49, and from the reports of the meeting of the R. A. S. in the *Astronomical Register* and *Observatory* for December, where it is mentioned that "Captain Maling discovered Comet *c* on the 14th of July, whereas Schäberle discovered it on the 15th of July," it would appear that there is here a claim to priority of discovery which, however, cannot be admitted on many grounds. Schäberle discovered it on the night of July 13, and determined the position as $= \alpha$, $5^h\ 44^m\ 59^s$, $\delta, +38°\ 37'$, at $14^h\ 47^m\ 30^s$, Washington mean time; whereas Captain Maling's alleged first observation was made on the evening of July 14.

It is evident that the Comet "of considerable magnitude" seen by Captain Maling in the constellation *Camelopardus* was not Schäberle's Comet at all, but Comet *b*, discovered by Tebbutt at Windsor, N. S. W., on May 22, which occupied a position*

* On July 14 at 13^h, the nucleus of Comet *b* was near the star P. IX. 37 (mag. 4·5), in the head of *Camelopardus* (α, $9^h\ 18^m\ 38^s$, $\delta, +81°\ 53'\ 18''$). Capt. Maling says the Comet he saw was about 20° from the horizon, directly under the polar star, which is impossible, because the altitude of *Polaris* at Grenada, West Indies, is only 12°.—W. F. D.

in the same region as that assigned to the object observed by Captain Maling. It could not possibly have been Schäberle's Comet, because it was far below the horizon at the time of Captain Maling's observations; and moreover this Comet was at the middle of July a very small telescopic object, about 1′ diameter, and, being immersed in the morning twilight, must have been utterly invisible with a small field-glass such as that used by Captain Maling. The facts clearly show that Comets *b* and *c* have been confused, and the former mistaken for the latter.

Ashley Down, Bristol:
1881, *December* 7.

Note on Silvering large Mirrors. By A. A. Common, Esq.

The anticipated difficulty of silvering large mirrors face downwards in the ordinary way, caused me to adopt the plan of silvering the mirror of my three-foot reflector face upwards, in the cell in which it had been made. At first a fairly good film of silver was got in this way, and a source of some anxiety was thought to be got over, as the removal of the silver at intervals of about one year was always contemplated; but subsequent attempts were not so successful, and I determined to try the other way.

As the plan I have designed and used for holding the mirror was quite successful, and is, I think, novel, I propose to describe it, especially as I have little doubt that the use of large reflectors will extend. After considering the various mechanical means that could be used to hold the mirror by the edge or back, or both, any of which would be open to the great objection that they might be causing strain of a most injurious kind, in a way that would not become apparent till perhaps too late, I decided to use the pressure of the atmosphere by the application of something in the nature of a large sucker, from which the air could be withdrawn to get the necessary hold on the back of the mirror, which in this case is about thirty-seven inches in diameter, four and a half inches thick, and weighs over four hundred pounds.

To carry out this idea, I had made a cast-iron box or cell, round in shape and about thirty inches diameter, the rim or side being four inches deep. The edge of this rim was turned quite flat and true, and grooved.

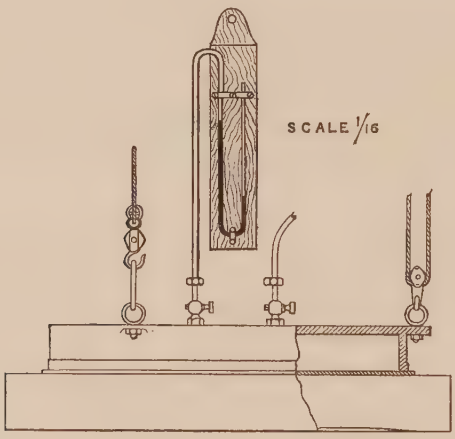
For the purpose of attaching the lifting gear three lugs were cast on the bottom, and iron eyes were fitted to them. Two small taps were fixed in the bottom of this box, to which could be attached flexible tubing.

One of these taps closed the connection of the box with a small vacuum gauge, made for the purpose, of quill glass tube in the shape of the letter U, each leg being about twelve inches long; mercury being poured in till it stood about half way up a rough scale on the board to which the gauge was fixed

enabled the pressure to be read off pretty closely. The other tap closed the connection of the box with the apparatus for withdrawing the air, which in the first case was my lungs; but, as there was not a back pressure valve, it was found convenient to use a small air-pump, such as is used with a seven-inch bell-glass; sufficient exhaustion could easily be got by the lungs to lift the mirror, with additional pressure for safety.

An indiarubber ring, about one and a half inch wide, and the diameter of the box, was provided to make the joint between the edge of box and the back of mirror.

The box itself might have been made much shallower, but I thought it advisable to have a precaution against the accidental breaking of the gauge or indiarubber tubes. The method of using this apparatus was as follows: the mirror was taken out of the cell and turned face down on a soft bed prepared for it, the

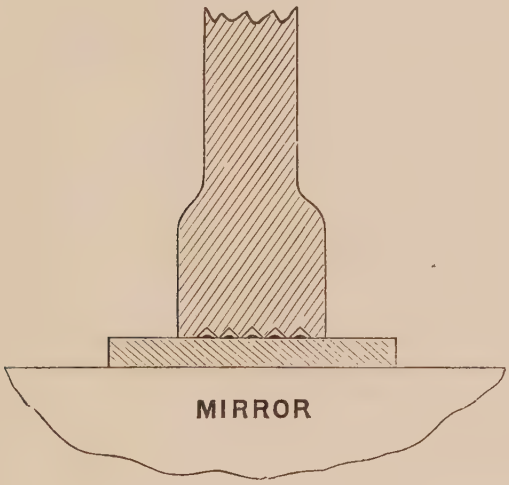


Side view of apparatus.

indiarubber ring placed central on the back, and the box on the ring. On sucking the air out of the box the pressure made a perfectly tight joint, and any desired degree of exhaustion could be made and kept. Two pulleys were hung from a beam in my workshop, one being attached to two of the eyes, and taking two-thirds of the weight, the other being attached to the other eye. By this arrangement the mirror could be put on edge or lowered flat or at the proper angle to enter the solution so as to drive out any air-bubbles or floating particles. All being ready, the pressure gauge, which was hung up over the box was made to register about five inches of mercury, and the mirror hoisted from its bed, turned on edge, cleaned, &c., all the operations being done in the same order as is usual in silvering smaller mirrors. The back of the mirror has a polished surface, yet the hold of the indiarubber under the pressure then used was very strong—

in fact, sufficient to hold the glass edge-up, in which position the tendency to slide off must have been very great. Of course, great care was taken to keep the water from getting on the back of the mirror and into the joint.

A sketch is given showing the arrangement, partly in section, that will complete the explanation. The application of this principle in a more perfect manner, especially for mirrors of comparative thinness, would naturally be to make the box large enough to hold the mirror, the rim acting as a support to the mirror when edgeways, and coming up to about one-half the thickness of edge, when the mirror back was close to bottom of box; then, by making a joint between the edge of mirror and rim of box by a flexible band in such a way that the mirror



Section showing joint.—Full size.

would be free to move up or down for a short distance, in turning the whole over the pressure of air, as soon as the mirror had dropped out of the box the little necessary distance, would exactly balance or support the whole surface equally. A further extension of the idea would be to make the permanent cell that is always provided to contain the levers and plates act as the box would do, and so always be ready without shifting the mirror; and this plan I shall certainly use if I ever make a larger telescope.

Several different processes of silvering were tried, but good results could not be obtained, perhaps because the temperature at the time I made the trials was very low, being from 50° to 56° F., or from the reducing agent not being correctly made.

After some experiments, I found the following process answer

very well, and as the solutions seem to keep well, and are easily made, it may be worth mentioning.

Separate solutions were made of nitrate of silver and caustic potash in the proportion of 1 oz. of each to 10 oz. of water. For the reducing agent glucose was used, and a solution of one half oz. in 10 ozs. of water was made. These three solutions, or any quantity in the same proportion, with liq. ammoniæ and distilled water being ready, the proper quantity was determined on upon the basis that the above quantities would be sufficient for 250 square inches of surface, and used in the following way:—

Ammonia was added to the solution of silver till the turbid appearance first produced had quite cleared; the potash solution was then added, and ammonia again added till the mixture was clear. Then a weak solution of silver was added, drop by drop, till the appearance was decidedly turbid again. The mirror which, before beginning, had been lowered into the dish already containing the proper amount of distilled water, was now lifted up, and the above mixture, together with the glucose solution, poured in and stirred well, and the mirror carefully lowered. At a temperature of about 56° a fine film was got in 43 minutes on the three-foot mirror. I have used some of the same solutions at various times to silver small surfaces, and find I can get a good film in much less time, particularly if the temperature is a little higher. No doubt for higher temperatures some modifications would have to be made, but the use of glucose allows a more certain determination of the proportions proper for certain temperatures than any mixture of sugar and acid, the active properties of which as a reducing agent are uncertain and changeable.

Ealing:
1881, *December* 8.

On a new Form of Transit-Circle with a Prismatic Object-Glass.

By E. J. Stone, M.A., F.R.S.

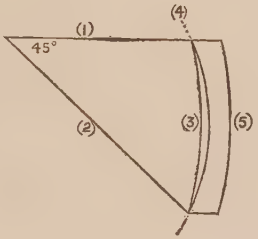
The proposed form is adapted either for Transit-Circle or Altazimuth, and it appears to me to offer many advantages. I am quite sensible of the difficulties of securing good prisms, but I think they are not insuperable. I have been in communication with Mr. Grubb on the subject, and I hope to give an object-glass of this description a fair trial as soon as Mr. Grubb is sufficiently relieved from the pressure of work in connection with the equipments of the Transit of *Venus* expeditions to undertake the manufacture of the object-glass.

I take for the calculations the following indices of refraction; but others can easily be adopted, if known, for the glass to be used:—

	Crown Glass.	Flint Glass.
D	1·52959	1·63504
E	1·53301	1·64202
F	1·53605	1·64826

The focal length of the combination is about 100 inches (99·972 inches) The following are the radii of curvatures, crown glass in front :—

Surface (1) sensibly plane
(2) „
(3) 21·928 inches
(4) 21·418 „
(5) 40·967 „

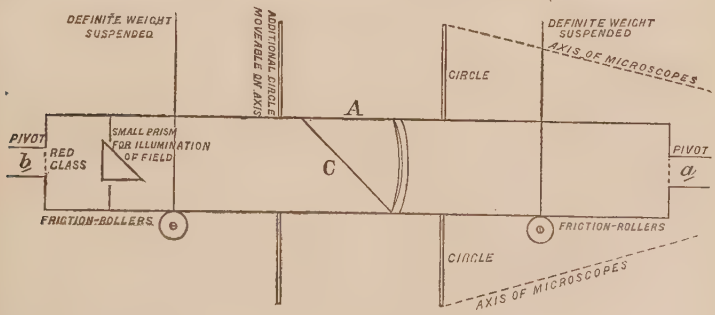


The spherical aberration is sensibly destroyed for light of the refrangibility of E.

The focal lengths are

D	99·973	99·972
E	99·968	
F	99·972	
C	100·028	

It is therefore an an achromatic object-glass for D, E, F. C stands out; but it occurs to me that if the wires were illuminated by red light through the opposite face of the prism, there would



be better definition and practically no loss of light. The only light really lost by the illumination of the wires would be that outstanding about C.

The surfaces A and C should be sensibly plane, but, as they turn with the whole optical sytem, exact geometrical planes are not necessary for accuracy.

The eyepiece joins on at *a*, and can be partially supported by friction-rollers with a definite weight suspended to avoid flexure. The whole turns about the pivots *a* and *b*, and should be reversible. The adjustments can all be determined as for an ordinary Transit-Circle, but reflexions near the zenith would be possible.

Radiant Points of Shooting Stars observed at Bristol in the Years 1878 *and* 1879. By W. F. Denning, Esq.

In continuation of my previous catalogues of meteor showers (*Monthly Notices*, Jan. 1877 and March 1878), I send two further lists, of 20 Radiants observed between July 21 and Aug. 10, 1878, and of 47 observed between July 28 and Nov. 14, 1879. I have selected the positions of such showers as were best observed, for in observations of this character there are always a large proportion of suspected streams from which four or five meteors only have been recorded. These have been omitted in the present case—indeed, it is necessary that uncertain positions should be invariably excluded, because they only originate complications which it is most desirable to avoid.

Towards the end of July and early in August 1878 we had a succession of very clear nights, and I recorded 621 shooting stars in 34 hours of observation. Amongst the large number of radiant points resulting from their reduction, two were of very exceptional character, and have already been referred to in the *Monthly Notices* (Jan. 1880, pp. 124–127). The following list comprises the most important streams :—

Radiant Points observed July 21 to August 10, 1878.

Reference No.	Date.	Radiant Point α	Radiant Point δ	No. of ☄'s	Notes and Comparisons.
1873.		°	°		
1	July 31–August 1	332	+50	14	Lacertids. Meteors very swift and short.
2	July 27–30	341	−13	54	δ Aquariads. Slowish, long meteors; fine shower. =No. 59, 1877.
3	July 30–August 1	32	+53	63	χ Perseids. Swift meteors, with streaks; very fine shower.
4	July 31–August 1	12	+70	16	Meteors faint and not very swift. Heis 15° + 70°, July 28–29.
5	,,	321	+31	10	Maximum, August 1. Very slow; near ζ Cygni.
6	August 1–2	291	+70	14	δ Draconids. Swift, short meteors. Heis 292° + 70°, August 3–19.
7	July 25–31 & Aug. 10	6	+37	23	Swift, streak-leaving meteors. =No. 49, 1877.
8	July 29–August 2	333	+ 9	10	Meteors very slow. Corder 333° + 12°, July.
9	July 21–August 1	11	+47	26	Meteors very swift, with streaks. Radiant diffuse. Probably two showers close together.
		12	+52		
10	July 25–26	332	+37	11	Bright, slow meteors, with long paths. S. & Z. 336° + 30°, July 28.
11	July 26–August 1	28	+36	12	Very swift meteors, with streaks. S. & Z. 32° + 35°, July 30.
12	July 26–31	333	+18	8	Swift, faint meteors. Corder 333° + 12°, July.
13	July 26–27	354	+42	7	Slowish, faint. Well defined.
14	July 31–August 1	7	+11	9	Meteors bright and very swift, with streaks. Radiant exact.
15	July 29–August 1	23	+41	7	Swift, streak-leaving meteors.
16	July 20–28	18	+59	8	Swift, streak-leaving meteors. Corder 20° + 60°, August.
17	July 27–31	28	+28	7	Swift meteors.
18	July 31–August 1	65	+60	7	Small, faint, and slow moving.
19	July 27–31	332	+27	8	Faint; very swift. S. & Z. 336° + 30°, July 28.
20	August 7–10	42½	+54	106	Perseids. Apparently a double shower, separated 5° in declination.
		44	+59		

In addition to these I may mention showers at 96°+72°, 47°+25°, 31°+18°, 305°−15°, 50°+75°, 33°−20°, 134°+78° 284°+44°, 22°+13°, 331°+62°, 76°+54°, &c., as observed with less distinctness. Indeed, though I have selected only 20 centres for the above table, I saw 41 different showers during the seven nights July 26–Aug. 2; and from a careful analysis of the path directions, there were indications of at least 14 additional radiants, so that the aggregate number of meteor streams in action at this particular epoch was fully 55. It is true that the end of July is rather a special period, but the number of visible streams (chiefly remarkable for their extreme feebleness) is very large on every night of the year. During my observations I recorded the number of meteors seen each night, together with the state of sky, duration of watch, &c. The figures show an exceptional abundance of shooting stars:—

1878.	Period of Observations.		Length.	☄'s seen.	State of Sky.
	h	h	h		
July 26	10¾ to	13	1½	27	Many clouds.
27	10½	14½	4	93	Very clear.
28	10½	14½	4	44	Haze; stars dim.
30	10½	14½	4	76	A little haze.
31	10	14½	4½	94	Very clear.
Aug. 1	9¾	12	2¼	42	Very clear. Cloudy after midnight.
2	10	11½	1½	20	Clear. Cloudy after 11½h.

Thus, during the period July 26 to August 2, 1878, I noted 403 shooting stars in watches extending in the aggregate over 22 hours.* This number was counted, notwithstanding the fact that, while registering the apparent paths of such as were accurately observed, many others must have escaped observation. On the nights of July 27 and 31, the horary rate for one observer, persistently looking eastwards, exceeded 30, and on the intervening nights I believe the numbers were equally large, but there was a good deal of haze which effectually obscured many of the smaller meteors. In 1879, I partially confirmed my results of the preceding year, for on July 28 in a two-hours watch between $11^h\ 45^m$ and $13^h\ 45^m$, I counted 45 meteors, of which several belonged to the rich showers of Aquariads and Perseids seen in 1878. In 1881, July 27, I also noted a remarkable frequency of shooting stars, but, being engaged in telescopic observations, their numbers and directions were not fully recorded on that occasion.

Observations were resumed at the end of July 1879, and continued until the middle of November, when ill health necessitated a suspension of the work. 1096 shooting stars had

* These figures include 7 meteors seen in a short interval on July 29.

been observed in 86 hours of watching. The results are grouped into five periods as follows:—

1879.	Hours of Observation.	Meteors Seen.	Radiants.
July 28–Aug. 12	$8\frac{1}{4}$	143	5
August 21–25	16	225	12
September 14–25	19	270	13
October 14–20	$24\frac{1}{2}$	280	11
November 12–14	$18\frac{1}{2}$	178	6

Only the most active radiants are mentioned. In the following table 47 positions are given, and to these 601 meteors were found to be conformable, or nearly 13 to each centre. Many of these showers correspond with those observed in 1877, and I have pointed out these agreements in cases where they are very close.

The chief shower observed in 1879 was on Aug. 21–25 (No. 6), from a point at 291°+60° (56 *l s*), near *o Draconis*. I gave a diagram of the tracks and accompanied it with a detailed account of the shower in the *Monthly Notices*, Jan. 1880, pp. 127–128; I also noted a very active stream (No. 32) in October from a point in the south of *Aries*, 31°+9°, which I have described in the same number of the *Notices*. As to the other displays, I saw 38 Perseids on Aug. 9–12 from the usual radiant north of *η Persei*, and in October the Orionids (No. 33) were well seen. On November 13 I watched the sky continuously for $11\frac{1}{2}$ hours ($5^h\ 30^m$ to 17^h), and counted 100 shooting stars; of these 18 were fine Leonids, nearly all of which made their apparition between $14\frac{1}{2}^h$ and $15\frac{1}{2}^h$. The apparent paths were noted with extreme care, and I found the radiant to be very sharply defined at 148°+23° (No. 42). Two showers (Nos. 28–31 and 18–39) in *Auriga*, separated 23° in declination, were sharply defined both in September and October from the points 77°+57° and 76°+33°, and I had seen the latter well both in October and November 1877 from the same point. The radiant immediately N. of *β Trianguli* (No. 19) in September was one of the best seen during the year; and the point of divergence is identical with one I have several times noted at the end of July and during the first half of August. I saw 25 Taurids and 19 Muscids on the nights of Nov. 12–14. They form active showers of slowish trained meteors, and are notable on account of their contemporary occurrence with the Leonids. They often supply meteors of considerable brilliancy, and will probably be frequently re-observed in future years, as two of the finest showers visible in November.

Radiant Points observed July 28 to November 14, 1879.

Reference No.	Date.	Radiant Point. α	δ	No. of ↓'s	Notes and Comparisons.
1879.	1879.	h	°		
1	July 28	338	−14	14	δ Aquariads. Slowish, long bright meteors. = Greg. 109.
2	July 28–29	32	+53	9	χ Perseids or Perseids II. Swift; streaks.
3	July 29	30	+37	6	Swift; streaks. A well-defined shower.
4	August 9–12	46	+58	38	Perseids. Meteors very swift, with streaks.
5	9–12	215	+76		Several bright meteors from a radiant here.
6	21–25	291	+60	56	Draconids. A fine shower of bright, slowish meteors.
7	21–23	46	+47	9	α Perseids. = No. 63, 1877.
8	21–25	62	+35	10	ε Perseids Meteors, swift with streaks.
9	21–23	61	+50	11	μ Perseids. = No. 83, 1877.
10	21–23	319	+30	12	Slow. At ξ Cygni. G. & H. 315° + 31°, July 4–August 22.
11	August 23	343	+14	6	Bright, swift meteors. Sawyer 348° + 17°, August 28–September 2.
12	August 21–25	339	−10	16	Slow, bright meteors. Aquariads. See No. 1. = No. 59, 1877.
13	21–23	70	+66	9	Swift meteors. Radiant close to *c* Camelopardi.
14	August 23	350	+47	8	Rather slow. Well defined and exact.
15	August 21–23	266	+47	7	Max. August 22. Slow, trained meteors.
16	21–23	5	+17	8	Slow meteors. Tupman 7° + 13°, August 18, 1869.
17	21–25	24	+42	9	Swift, bright meteors, with streaks. At γ Andromedæ.

18	September 14–25	76 + 32	10	Swift, with streaks. Schmidt 70° + 32°, September. See No. 39.
19	15–25	30 + 36	16	Very sharply defined. Meteors not very swift. S. & Z. 28° + 35°, Sept. 23.
20	14–25	99 + 43	15	Very swift, with streaks. Seen also on Oct. 15 and 20 at 105° + 50°.
21	14–21	76 + 44	11	α Aurigids. Swift and short, with streaks.
22	14–25	31 + 19	14	Max. September 21, 10 $\swarrow$'s. Slowish. In Aries.
23	14–25	82 + 75	8	Not swift; streaks. Radiant exact.
24	20–21	192 + 79	9	Very, very slow. 8° *p.* β Ursæ Minoris.
25	15–25	355 + 18	6	Slowish, bright meteors. G. & H. 352° + 17°, August 22–October 15.
26	14–25	61 + 48	8	Swift, no streaks. At μ Persei.
27	14–21	87 + 43	8	β Aurigids. } Meteors, swift with streaks.
28	14–21	76 + 56	7	δ Aurigids. } Beginning of No. 31.
29	14–21	50 + 54	7	Rapid, no streaks. Also at 31° + 52° (5 slow $\swarrow$'s).
30	Sept. 21–Oct. 20	84 − 11	7	Swift, streaks. Tupman 85° − 15°, August 31, 1870.
31	October 14–20	78 + 57	12	δ Aurigids. Not very swift; streaks.
32	14–20	31 + 9	37	Max. October 15, 21 $\swarrow$'s. An active shower of slow and generally small meteors. Corder 32° + 11°, Oct. Tupman 28° + 10°, Oct. 13.
33	October 15 & 20	93 + 17	39	Orionids. Very swift and short, invariably leaving streaks.
34	15 & 20	106 + 23	14	Very swift, long meteors, with streaks. Tupman 105° + 24°, Oct. 12, 1869.

Reference No.	Date.	Radiant Point. α δ	No. of ↓'s	Notes and Comparisons.
1879.	1879.	° °		
35	October 4–6 & 16–20	316+59	17	Meteors quick, rather bright. = Heis, B 9, 315°+59°, Oct. 3–Nov. 13.
36	October 14–20	95+46	11	Max. October 14. Meteors swift, with streaks. = No. 97, 1877.
37	15 & 20	7+51	11	Max. Oct. 15. Slow and faint. Just S. of α Cassiopeiæ. = No. 137, 1877.
38	October 20	45+6	8	Meteors slow. N. of α Eridani. Tupman 44°+4°, October 14, 1869.
39	October 14–15	76+33	7	Not very swift. = No. 126, 1877. Tupman 77°+30°, October 13, 1869.
40	15 & 20	114+62	7	Meteors bright, very swift and streak-leaving.
41	15 & 20	70+65	7	Meteors rather slow. At *c* Camelopardi. = No. 34, 1876.
42	November 13	148+23	18	Leonids. Meteors brilliant, with streaks. A contemporary shower seen at 125°+55° (5↓'s).
43	November 12–13	133+70	8	Rapid meteors, without streaks.
44	November 12	62+21½	14	Slow. } Showers of Taurids. Meteors brilliant, with trains of ashy sparks.
45	November 13–14	58+21	11	Slow. }
46	12–14	46+21	19	Muscids. Max. November 13. Meteors slow, with spark-trails.
47	12–14	80+24	7	Meteors not rapid. Beginning of Taurids II.

Ashley Down, Bristol:
1881, *Nov.* 15.